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FRIDAY, OCTOBER 1, 1897.

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MSS. intended for publication and books, etc., intended for review should be sent to the responsible editor, Prof. J. McKeen Cattell, Garrison-on-Hudson, N. Y.

AN UNDISCOVERED GAS.*

A SECTIONAL address to members of the British Association falls under one of three heads. It may be historical, or actual, or

* Address to the Chemical Section, British Association for the Advancement of Science, Toronto, 1897, by the President of the Section.

prophetic; it may refer to the past, the present or the future. In many cases, indeed in all, this classification overlaps. Your former presidents have given sometimes a historical introduction, followed by an account of the actual state of some branch of our science, and, though rarely, concluding with prophetic remarks. To those who have an affection for the past, the historical side appeals forcibly; to the practical man, and to the investigator engaged in research, the actual, perhaps, presents more charm; while to the general public, to whom novelty is often more of an attraction than truth, the prophetic aspect excites most interest. In this address I must endeavor to tickle all palates; and perhaps I may be excused if I take this opportunity of indulging in the dangerous luxury of prophecy, a luxury which the managers of scientific journals do not often permit their readers to taste.

The subject of my remarks to-day is a new gas. I shall describe to you later its curious properties; but it would be unfair not to put you at once in possession of the knowledge of its most remarkable property—it has not yet been discovered. As it is still unborn, it has not yet been named. The naming of a new element is no easy matter. For there are only twenty-six letters in our alphabet, and there are already over seventy elements. To select a name expressible by a symbol which has not al-

ready been claimed for one of the known elements is difficult, and the difficulty is enhanced when it is at the same time required to select a name which shall be descriptive of the properties (or want of properties) of the element.

It is now my task to bring before you the evidence for the existence of this undiscovered element.

It was noticed by Döbereiner, as long ago as 1817, that certain elements could be arranged in groups of three. The choice of the elements selected to form these triads was made on account of their analogous properties, and on the sequence of their atomic weights, which had at that time only recently been discovered. Thus calcium, strontium and barium formed such a group; their oxides, lime, strontia and baryta are all easily slaked, combining with water to form soluble lime-water, strontia-water and baryta-water. Their sulphates are all sparingly soluble and resemblance had been noticed between their respective chlorides and between their nitrates. Regularity was also displayed by their atomic weights. The numbers then accepted were 20, 42.5 and 65; and the atomic weight of strontium, 42.5, is the arithmetical mean of those of the other two elements, for $(65+20)/2 = 42.5$. The existence of other similar groups of three was pointed out by Döbereiner, and such groups became known as 'Döbereiner's triads.'

Another method of classifying the elements, also depending on their atomic weights, was suggested by Pettenkofer, and afterwards elaborated by Kremers, Gladstone and Cooke. It consisted in seeking for some expression which would represent the differences between the atomic weights of certain allied elements. Thus, the difference between the atomic weight of lithium, 7, and sodium, 23, is 16; and between that of sodium and of potassium, 39, is also 16. The regularity is not always so conspicu-

ous; Dumas, in 1857, contrived a somewhat complicated expression, which, to some extent, exhibited regularity in the atomic weights of fluorine, chlorine, bromine, and iodine, and also of nitrogen, phosphorus, arsenic, antimony and bismuth.

The upshot of these efforts to discover regularity was that, in 1864, Mr. John Newlands, having arranged the elements in eight groups, found that when placed in the order of their atomic weights, 'the eighth element, starting from a given one, is a kind of repetition of the first, like the eighth note of an octavo in music.' To this regularity he gave the name 'The Law of Octaves.'

The development of this idea, as all chemists know, was due to the late Professor Lothar Meyer, of Tübingen, and to Professor Mendeléeff, of St. Petersburg. It is generally known as the 'Periodic Law.' One of the simplest methods of showing this arrangement is by means of a cylinder divided into eight segments by lines drawn parallel to its axis; a spiral line is then traced round the cylinder, which will, of course, be cut by these lines eight times at each revolution. Holding the cylinder vertically, the name and atomic weight of an element is written at each intersection of the spiral with a vertical line, following the numerical order of the atomic weights. It will be found, according to Lothar Meyer and Mendeléeff, that the elements grouped down each of the vertical lines form a natural class; they possess similar properties, form similar compounds, and exhibit a graded relationship between their densities, melting points, and many of their other properties. One of these vertical columns, however, differs from the others, inasmuch as on it there are three groups, each consisting of three elements with approximately equal atomic weights. The elements in question are iron, cobalt and nickel; palladium, rhodium and ruthenium; and platinum,

iridium and osmium. There is apparently room for a fourth group of three elements in this column, and it may be a fifth. And the discovery of such a group is not unlikely, for when this table was first drawn up Professor Mendeléeff drew attention to certain gaps, which have since been filled up by the discovery of gallium, germanium and others.

The discovery of argon at once raised the curiosity of Lord Rayleigh and myself as to its position in this table. With a density of nearly 20, if a diatomic gas, like oxygen and nitrogen, it would follow fluorine in the periodic table; and our first idea was that argon was probably a mixture of three gases, all of which possessed nearly the same atomic weights, like iron, cobalt and nickel. Indeed, their names were suggested, on this supposition, with patriotic bias, as Anglium, Scotium and Hibernium! But when the ratio of its specific heats had, at least in our opinion, unmistakably shown that it was molecularly monatomic, and not diatomic, as at first conjectured, it was necessary to believe that its atomic weight was 40, and not twenty, and that it followed chlorine in the atomic table and not fluorine. But here arises a difficulty. The atomic weight of chlorine is 35.5, and that of potassium, the next element in order in the table, is 39.1; and that of argon, 40, follows, and does not precede, that of potassium, as it might be expected to do. It still remains possible that argon, instead of consisting wholly of monatomic molecules, may contain a small percentage of diatomic molecules; but the evidence in favor of this supposition is, in my opinion, far from strong. Another possibility is that argon, as at first conjectured, may consist of a mixture of more than one element; but, unless the atomic weight of one of the elements in the supposed mixture is very high, say 82, the case is not bettered, for one of the elements in the supposed trio would

still have a higher atomic weight than potassium. And very careful experiments, carried out by Dr. Norman Collie and myself, on the fractional diffusion of argon, have disproved the existence of any such element with high atomic weight in argon, and, indeed, have practically demonstrated that argon is a simple substance and not a mixture.

The discovery of helium has thrown a new light on this subject. Helium, it will be remembered, is evolved on heating certain minerals, notably those containing uranium; although it appears to be contained in others in which uranium is not present, except in traces. Among these minerals are clèveite, monazite, fergusonite, and a host of similar complex mixtures, all containing rare elements, such as niobium, tantalum, yttrium, cerium, etc. The spectrum of helium is characterized by a remarkably brilliant yellow line, which had been observed as long ago as 1868 by Professors Frankland and Lockyer in the spectrum of the sun's chromosphere, and named 'helium' at that early date.

The density of helium proved to be very close to 2.0, and, like argon, the ratio of its specific heat showed that it, too, was a monatomic gas. Its atomic weight, therefore, is identical with its molecular weight, viz., 4.0, and its place in the periodic table is between hydrogen and lithium, the atomic weight of which is 7.0.

The difference between the atomic weights of helium and argon is thus 36, or $40 - 4$. Now there are several cases of such a difference. For instance, in the group the first member of which is fluorine we have:

Fluorine.....	19	16.5
Chlorine.....	35.5	19.5
Manganese.....	55	

In the oxygen group—

Oxygen.....	16	16
Sulphur.....	32	20.3
Chromium.....	52.3	

In the nitrogen group—

Nitrogen	14	17
Phosphorus	31	20.4
Vanadium.....	51.4	

And in the carbon group—

Carbon.....	12	16.3
Silicon	28.3	19.8
Titanium	48.1	

These instances suffice to show that approximately the differences are 16 and 20 between consecutive members of the corresponding groups of elements. The total differences between the extreme members of the short series mentioned are—

Manganese—Fluorine.....	36
Chromium—Oxygen.....	36.3
Vanadium—Nitrogen	37.4
Titanium—Carbon	36.1

This is approximately the difference between the atomic weights of helium and argon, 36.

There should, therefore, be an undiscovered element between helium and argon, with an atomic weight 16 units higher than that of helium, and 20 units lower than that of argon, namely 20. And if this unknown element, like helium and argon, should prove to consist of monatomic molecules, then its density should be half its atomic weight, 10. And pushing the analogy still farther, it is to be expected that this element should be as indifferent to union with other elements as the two allied elements.

My assistant, Mr. Morris Travers, has indefatigably aided me in a search for this unknown gas. There is a proverb about looking for a needle in a haystack; modern science, with the aid of suitable magnetic appliances, would, if the reward were sufficient, make short work of that proverbial needle. But here is a supposed unknown gas, endowed, no doubt, with negative properties and the whole world to find it in. Still, the attempt had to be made.

We first directed our attention to the

sources of helium—minerals. Almost every mineral which we could obtain was heated in a vacuum, and the gas which was evolved examined. The results are interesting. Most minerals give off gas when heated, and the gas contains, as a rule, a considerable amount of hydrogen, mixed with carbonic acid, questionable traces of nitrogen, and carbonic oxide. Many of the minerals, in addition, gave helium, which proved to be widely distributed, though only in minute proportion. One mineral—malacone—gave appreciable quantities of argon; and it is noteworthy that argon was not found except in it (and, curiously, in much larger amount than helium), and in a specimen of meteoric iron. Other specimens of meteoric iron were examined, but were found to contain mainly hydrogen, with no trace of either argon or helium. It is probable that the sources of meteorites might be traced in this manner, and that each could be relegated to its particular swarm.

Among the minerals examined was one to which our attention had been directed by Professor Lockyer, named eliasite, from which he said that he had extracted a gas in which he had observed spectrum lines foreign to helium. He was kind enough to furnish us with a specimen of this mineral, which is exceedingly rare, but the sample which we tested contained nothing but undoubted helium.

During a trip to Iceland, in 1895, I collected some gas from the boiling springs there; it consisted, for the most part, of air, but contained somewhat more argon than is usually dissolved when air is shaken with water. In the spring of 1896 Mr. Travers and I made a trip to the Pyrenees to collect gas from the mineral springs of Caunterets, to which our attention had been directed by Dr. Bouchard, who pointed out that these gases are rich in helium. We examined a number of samples from the

various springs, and confirmed Dr. Bouchard's results, but there was no sign of any unknown lines in the spectrum of these gases. Our quest was in vain.

We must now turn to another aspect of the subject. Shortly after the discovery of helium its spectrum was very carefully examined by Professors Runge and Paschen, the renowned spectroscopists. The spectrum was photographed, special attention being paid to the invisible portions, termed the 'ultra-violet' and 'infra-red.' The lines thus registered were found to have a harmonic relation to each other. They admitted of division into two sets, each complete in itself. Now, a similar process had been applied to the spectrum of lithium and to that of sodium, and the spectra of these elements gave only one series each. Hence, Professors Runge and Paschen concluded that the gas, to which the provisional name of helium had been given, was, in reality, a mixture of two gases, closely resembling each other in properties. As we know no other elements with atomic weights between those of hydrogen and lithium, there is no chemical evidence either for or against this supposition. Professor Runge supposed that he had obtained evidence of the separation of these imagined elements from each other by means of diffusion; but Mr. Travers and I pointed out that the same alteration of spectrum, which was apparently produced by diffusion, could also be caused by altering the pressure of the gas in the vacuum tube; and shortly after Professor Runge acknowledged his mistake.

These considerations, however, made it desirable to subject helium to systematic diffusion, in the same way as argon had been tried. The experiments were carried out in the summer of 1896, by Dr. Collie and myself. The result was encouraging. It was found possible to separate helium into two portions of different rates of diffusion, and consequently of different

density by this means. The limits of separation, however, were not very great. On the one hand, we obtained gas of a density close on 2.0; and on the other, a sample of density 2.4 or thereabouts. The difficulty was increased by the curious behavior, which we have often had occasion to confirm, that helium possesses a rate of diffusion too rapid for its density. Thus, the density of the lightest portion of the diffused gas, calculated from its rate of diffusion, was 1.874; but this corresponds to a real density of about 2.0. After our paper, giving an account of these experiments, had been published, a German investigator, Herr A. Hagenbach, repeated our work and confirmed our results.

The two samples of gas of different density differ also in other properties. Different transparent substances differ in the rate at which they allow light to pass through them. Thus, light travels through water at a much slower rate than through air, and at a slower rate through air than through hydrogen. Now Lord Rayleigh found that helium offers less opposition to the passage of light than any other substance does, and the heavier of the two portions into which helium had been split offered more opposition than the lighter portion. And the retardation of the light, unlike what has usually been observed, was nearly proportional to the densities of the samples. The spectrum of these two samples did not differ in the minutest particular; therefore it did not appear quite out of the question to hazard the speculation that the process of diffusion was instrumental, not necessarily in separating two kinds of gas from each other, but actually in removing light molecules of the same kind from heavy molecules. This idea is not new. It had been advanced by Prof. Schützenberger (whose recent death all chemists have to deplore), and later, by Mr. Crookes, that what we term the atomic weight of an

element is a mean ; that when we say that the atomic weight of oxygen is 16 we merely state that the average atomic weight is 16 ; and it is not inconceivable that a certain number of molecules have a weight somewhat higher than 32, while a certain number have a lower weight.

We therefore thought it necessary to test this question by direct experiment with some known gas ; and we chose nitrogen, as a good material with which to test the point. A much larger and more convenient apparatus for diffusing gases was built by Mr. Travers and myself, and a set of systematic diffusions of nitrogen was carried out. After thirty rounds, corresponding to 180 diffusions, the density of the nitrogen was unaltered, and that of the portion which should have diffused most slowly, had there been any difference in rate, was identical with that of the most quickly diffusing portion, *i. e.*, with that of the portion which passed first through the porous plug. This attempt, therefore, was unsuccessful ; but it was worth carrying out, for it is now certain that it is not possible to separate a gas of undoubted chemical unity into portions of different density by diffusion. And these experiments rendered it exceedingly improbable that the difference in density of the two fractions of helium was due to separation of light molecules of helium from heavy molecules.

The apparatus used for diffusion had a capacity of about two litres. It was filled with helium, and the operation of diffusion was carried through thirty times. There were six reservoirs, each full of gas, and each was separated into two by diffusion. To the heavier portion of one lot the lighter portion of the next was added, and in this manner all six reservoirs were successively passed through the diffusion apparatus. This process was carried out thirty times, each of the six reservoirs hav-

ing had its gas diffused each time thus involving 180 diffusions.

After this process the density of the more quickly diffusing gas was reduced to 2.02, while that of the less quickly diffusing had increased to 2.27. The light portion on re-diffusion hardly altered in density, while the heavier portion, when divided into three portions by diffusion, showed a considerable difference in density between the first third and the last third. A similar set of operations was carried out with a fresh quantity of helium, in order to accumulate enough gas to obtain a sufficient quantity for a second series of diffusions. The more quickly diffusing portions of both gases were mixed and re-diffused. The density of the lightest portion of these gases was 1.98, and after other 15 diffusions the density of the lightest portion had not decreased. The end had been reached ; it was not possible to obtain a lighter portion by diffusion. The density of the main body of this gas is therefore 1.98 ; and its refractivity, air being taken as unity, is 0.1245. The spectrum of this portion does not differ in any respect from the usual spectrum of helium.

As re-diffusion does not alter the density or the refractivity of this gas, it is right to suppose that either one definite element has now been isolated or that if there are more elements than one present they possess the same, or very nearly the same, density and refractivity. There may be a group of elements, say three, like iron, cobalt and nickel ; but there is no proof that this idea is correct and the simplicity of the spectrum would be an argument against such a supposition. This substance, forming by far the larger part of the whole of the gas, must, in the present state of our knowledge, be regarded as pure helium.

On the other hand, the heavier residue is easily altered in density by re-diffusion, and this would imply that it consists of a

small quantity of a heavy gas mixed with a large quantity of the light gas. Repeated re-diffusion convinced us that there was only a very small amount of the heavy gas present in the mixture. The portion which contained the largest amount of heavy gas was found to have the density 2.275, and its refractive index was found to be 0.1333. On re-diffusing this portion of gas until only a trace sufficient to fill a Plücker's tube was left, and then examining the spectrum, no unknown lines could be detected, but, on interposing a jar and spark gap, the well-known blue lines of argon became visible; and even without the jar the red lines of argon and the two green groups were distinctly visible. The amount of argon present, calculated from the density, was 1.64 per cent., and from the refractivity 1.14 per cent. The conclusion had therefore to be drawn that the heavy constituent of helium, as it comes off the minerals containing it, is nothing new, but, so far as can be made out, merely a small amount of argon.

If, then, there is a new gas in what is generally termed helium, it is mixed with argon, and it must be present in extremely minute traces. As neither helium nor argon has been induced to form compounds, there does not appear to be any method, other than diffusion, for isolating such a gas, if it exists, and that method has failed in our hands to give any evidence of the existence of such a gas. It by no means follows that the gas does not exist; the only conclusion to be drawn is that we have not yet stumbled on the material which contains it. In fact, the haystack is too large and the needle too inconspicuous. Reference to the periodic table will show that between the elements aluminium and indium there occurs gallium, a substance occurring only in the minutest amount on the earth's surface; and following silicon, and preceding tin, appears the element germanium, a body

which has as yet been recognized only in one of the rarest of minerals, argyrodite. Now, the amount of helium in fergusonite, one of the minerals which yields it in reasonable quantity, is only 33 parts by weight to 100,000 of the mineral; and it is not improbable that some other mineral may contain the new gas in even more minute proportion. If, however, it is accompanied in its still undiscovered source by argon and helium, it will be a work of extreme difficulty to effect a separation from these gases.

In these remarks it has been assumed that the new gas will resemble argon and helium in being indifferent to the action of reagents, and in not forming compounds. This supposition is worth examining. In considering it the analogy with other elements is all that we have to guide us.

We have already paid some attention to several triads of elements. We have seen that the differences in atomic weights between the elements fluorine and manganese, oxygen and chromium, nitrogen and vanadium, carbon and titanium, is in each case approximately the same as that between helium and argon, viz., 36. If elements further back in the periodic table be examined, it is to be noticed that the differences grow less, the smaller the atomic weights. Thus, between boron and scandium, the difference is 33; between beryllium (glucinum) and calcium, 31; and between lithium and potassium, 32. At the same time, we may remark that the elements grow liker each other the lower the atomic weights. Now helium and argon are very like each other in physical properties. It may be fairly concluded, I think, that in so far they justify their position. Moreover, the pair of elements which show the smallest difference between their atomic weights is beryllium and calcium; there is somewhat greater difference between lithium and potassium. And, it is in accordance

with this fragment of regularity that helium and argon show a greater difference. Then, again, sodium, the middle element of the lithium triad, is very similar in properties both to lithium and potassium; and we might, therefore, expect that the unknown element of the helium series should closely resemble both helium and argon.

Leaving now the consideration of the new element, let us turn our attention to the more general question of the atomic weight of argon, and its anomalous position in the periodic scheme of the elements. The apparent difficulty is this: The atomic weight of argon is 40; it has no power to form compounds, and thus possesses no valency; it must follow chlorine in the periodic table, and precede potassium; but its atomic weight is greater than that of potassium, whereas it is generally contended that the elements should follow each other in the order of their atomic weights. If this contention is correct, argon should have an atomic weight smaller than 40.

Let us examine this contention. Taking the first row of elements, we have:

Li=7, Be=9.8, B=11, C=12, N=14, O=16,
F=19, ?=20.

The differences are:

2.8, 1.2, 1.0, 2.0, 2.0, 3.0, 1.0.

It is obvious that they are irregular. The next row shows similar irregularities. Thus:

(?=20), Na=23, Mg=24.3, Al=27, Si=28, P=31,
S=32, Cl=35.5, A=40.

And the differences:

3.0, 1.3, 2.7, 1.0, 3.0, 1.0, 3.5, 4.5.

The same irregularity might be illustrated by a consideration of each succeeding row. Between argon and the next in order, potassium, there is a difference of -0.9 ; that is to say, argon has a higher atomic weight than potassium by 0.9 unit; whereas it might be expected to have a lower one, seeing that potassium follows

argon in the table. Farther on in the table there is a similar discrepancy. The row is as follows:

Ag=108, Cd=112, In=114, Sn=119, Sb=120.5,
Te=127.7, I=127.

The differences are:

4.0, 2.0, 5.0, 1.5, 7.2, -0.7 .

Here, again, there is a negative difference between tellurium and iodine. And this apparent discrepancy has led to many and careful redeterminations of the atomic weight of tellurium. Professor Brauner, indeed, has submitted tellurium to methodical fractionation, with no positive results. All the recent determinations of its atomic weight give practically the same number, 127.7.

Again, there have been almost innumerable attempts to reduce the differences between the atomic weights to regularity, by contriving some formula which will express the numbers which represent the atomic weights, with all their irregularities. Needless to say, such attempts have in no case been successful. Apparent success is always attained at the expense of accuracy, and the numbers reproduced are not those accepted as the true atomic weights. Such attempts, in my opinion, are futile. Still, the human mind does not rest contented in merely chronicling such an irregularity; it strives to understand why such an irregularity should exist. And, in connection with this, there are two matters which call for our consideration. These are: Does some circumstance modify these 'combining proportions' which we term 'atomic weights'? And is there any reason to suppose that we can modify them at our will? Are they true 'constants of Nature' unchangeable, and once for all determined? Or are they constant merely so long as other circumstances, a change in which would modify them, remain unchanged.

In order to understand the real scope of

such questions, it is necessary to consider the relation of the 'atomic weights' to other magnitudes, and especially to the important quantity termed 'energy.'

It is known that energy manifests itself under different forms and that one form of energy is quantitatively convertible into another form, without loss. It is also known that each form of energy is expressible as the product of two factors, one of which has been termed the 'intensity factor,' and the other the 'capacity factor.' Professor Ostwald, in the last edition of his 'Allgemeine Chemie,' classifies some of these forms of energy as follows:

Kinetic energy is the product of Mass into the square of velocity.

Linear energy is the product of Length into force.

Surface energy is the product of Surface into surface tension.

Volume energy is the product of Volume into pressure.

Heat energy is the product of Heat-capacity (entropy) into temperature.

Electrical energy is the product of Electric capacity into potential.

Chemical energy is the product of 'Atomic weight' into affinity.

In each statement of factors, the 'capacity factor' is placed first, and the 'intensity-factor' second.

In considering the 'capacity factors,' it is noticeable that they may be divided into two classes. The two first kinds of energy, kinetic and linear, are *independent of the nature of the material* which is subject to the energy. A mass of lead offers as much resistance to a given force, or, in other words, possesses as great inertia as an equal mass of hydrogen. A mass of iridium, the densest solid, counterbalances an equal mass of lithium, the lightest known solid. On the other hand, surface energy deals with molecules, and not with masses. So does volume energy. The volume energy of two grammes of hydrogen, contained in a vessel of one litre capacity, is equal to

that of thirty-two grammes of oxygen at the same temperature and contained in a vessel of equal size. Equal masses of tin and lead have not equal capacity for heat; but 119 grammes of tin has the same capacity as 207 grammes of lead, that is, equal atomic masses have the same heat capacity. The quantity of electricity conveyed through an electrolyte under equal difference of potential is proportional, not to the mass of the dissolved body, but to its equivalent, that is, to some simple fraction of its atomic weight. And the capacity factor of chemical energy is the atomic weight of the substance subjected to the energy. We see, therefore, that while mass or inertia are important adjuncts of kinetic and linear energies all other kinds of energy are connected with atomic weights, either directly or indirectly.

Such considerations draw attention to the fact that quantity of matter (assuming that there exists such a carrier of properties as we term 'matter') need not necessarily be measured by its inertia, or by gravitational attraction. In fact, the word 'mass' has two totally distinct significations. Because we adopt the convention to measure quantity of matter by its mass, the word 'mass' has come to denote 'quantity of matter.' But it is open to anyone to measure a quantity of matter by any other of its energy factors. I may, if I choose, state that those quantities of matter which possess equal capacities for heat are equal, or that 'equal numbers of atoms' represent equal quantities of matter. Indeed, we regard the value of material as due rather to what it can do than to its mass; and we buy food, in the main, on an atomic, or, perhaps, a molecular basis, according to its content of albumen. And most articles depend for their value on the amount of food required by the producer or the manufacturer.

The various forms of energy may, there-

fore, be classified as those which can be referred to an 'atomic' factor, and those which possess a 'mass' factor. The former are in the majority. And the periodic law is the bridge between them; as yet, an imperfect connection. For the atomic factors, arranged in the order of their masses, display only a partial regularity. It is undoubtedly one of the main problems of physics and chemistry to solve this mystery. What the solution will be is beyond my power of prophecy; whether it is to be found in the influence of some circumstance on the atomic weights, hitherto regarded as among the most certain 'constants of Nature,' or whether it will turn out that mass and gravitational attraction are influenced by temperature, or by electrical charge, I cannot tell. But that some means will ultimately be found of reconciling these apparent discrepancies, I firmly believe. Such a reconciliation is necessary, whatever view be taken of the nature of the universe and of its mode of action; whatever units we may choose to regard as fundamental among those which lie at our disposal.

In this address I have endeavored to fulfill my promise to combine a little history, a little actuality and a little prophecy. The history belongs to the Old World; I have endeavored to share passing events with the New; and I will ask you to join with me in the hope that much of the prophecy may meet with its fulfilment on this side of the ocean.

WILLIAM RAMSAY.

*ADDRESS BY THE PRESIDENT BEFORE THE
SOCIETY FOR THE PROMOTION OF
ENGINEERING EDUCATION.*

IN opening the proceedings of this fourth annual meeting of the Society for the Promotion of Engineering Education I wish, first of all, to congratulate the Society upon its great success thus far in accomplishing

the object for which it was founded, an object fully expressed in its name. The volumes of its proceedings already published are filled with discussions and the ripe conclusions of the best thought that can be expressed to-day upon many phases of engineering education. No one who desires to become informed upon these matters can afford to neglect these volumes. It is believed that all interested in the object of our Society will find it to their advantage to unite with us.

One striking peculiarity of engineering education seems to me to lie in the fact that it has been determined so largely as to its scope and the lines of its development by the engineering colleges themselves in advance of the formulated demands of the engineering profession and of the public in general, and often, indeed, in opposition to such demands. Through the wisdom and foresight of these organizers of engineering education the profession of engineering has come forth during this generation into public estimation as a learned and responsible profession, quite the peer of law or medicine. This is the work of the engineering colleges, and from the deliberations of this Society it is evident that they still have a large work before them. The educational institutions of our country are in a state of flux. The present movement in education is powerful. These times will be looked back to in future days as those in which mighty educational forces were inaugurated and were adapted to the needs of the nation just as it was coming to its full consciousness as one of the great family of nations, a consciousness of power and responsibility that is causing it to depart somewhat from the revered advice of Washington, which was to keep aloof from European affairs and entanglements with other nations of the earth and work out its own destiny by itself. National growth and our multiplied facilities for communication have greatly modi-